Society Reports.

AMERICAN NEUROLOGICAL ASSOCIATION.

TWELFTH ANNUAL REPORT.

First day, morning session.

The American Neurological Association convened at the Howland House, Long Branch, N. J., July 21, 1886, and was called to order by the President, Dr. Burt G. Wilder, of Ithaca, N. Y., who delivered the address as retiring President, on "The Collocation of a Suture and Fissure in the Human Fœtus" (see JOURNAL OF NERVOUS AND MENTAL DISEASE, vol. xiii., p. 463).

At the close of his address the President introduced the President-elect, Dr. Charles K. Mills, of Philadelphia.

On motion, the reading of the minutes of the last annual meeting, already published, was dispensed with.

Dr. R. W. Amidon, of New York, Secretary and Treasurer, read the

REPORT OF THE TREASURER,

prepared by Dr. G. M. Hammond, of New York, Secretary and Treasurer for last year. On motion the report was accepted.

THE REPORT OF THE COUNCIL

was then read, and it was recommended that the following candidates be elected to active membership: James Hendrie Lloyd and F. X. Dercum, of Philadelphia; B. Sachs, J. Rudisch, and E. D. Fisher, of New York.

They were elected by an affirmative ballot cast by the Secretary.

MEMBERS PRESENT.

The following members were present during either the whole or a part of the meeting:

Drs. Burt G. Wilder, of Ithaca, N. Y.; Charles K. Mills, of

Philadelphia ; R. W. Amidon, of New York ; G. M. Hammond, of New York ; L. C. Gray, of Brooklyn ; Philip Zenner, of Cincinnati, O.; R. L. Parsons, of Sing Sing, N. Y.; John Van Bibber, of Baltimore, Md.; W. R. Birdsall, C. L. Dana, L. Weber, V. P. Gibney, E. D. Fisher, S. J. McNutt, B. Sachs, and G. W. Jacoby, of New York ; F. X. Dercum, J. H. Lloyd, G. B. Massey, and W. Sinkler, of Philadelphia.

The President, Dr. Charles K. Mills, of Philadelphia, after the transaction of the above preliminary business, delivered his Inaugural Address. (See JOURNAL OF NERVOUS AND MENTAL DISEASE, vol. xiii., p. 517.)

Remarks on the President's Address.

Dr. BURT G. WILDER, of Ithaca, N. Y.: Inasmuch as you have asked me to open the discussion on the paper, I will do so to a slight extent, although there are some parts of it on which I do not feel competent to make any remarks.

The subject I regard as one in every way worthy of the importance which our President has assigned to it as the subject of his inaugural address, and I cannot but feel that it is substantial encouragement in the way of exact and philosophical, and, in the highest sense, scientific methods of studying neurology and psychiatry together. I feel very sure that there are more psychologists than I have had the misfortune to meet, who do not think it necessary to study the brain at all. And, on the other hand, important as is the subject, there are practising physicians and surgeons who are not so familiar with this organ as they should be.

It seems to me that the highest compliment that our President could pay to this Association is in the straightforward way which he has adopted of telling us these facts with regard to these brains, as if he took it for granted that we would know what he meant. I venture to say that not in every gathering of practitioners in the United States would it have been possible to present a paper of this kind and have it at all intelligible to the hearers.

Personally I feel an intense interest in all that he has said, and I only wish that I had had opportunity to examine the brains more fully in order to confirm some of his statements, and I trust, with some diffidence, to object to some others.

In the first place, he has called attention to the exposure of the

insula in the negro's brain. In the only negro's brain which I have, the insula was exposed much more than in any of the brains showed to-day. I speak of it because it was hardened within the skull, and there was no possibility of an abnormal covering or uncovering from exposure.

In the Chinaman's brain it will be noticed that the insula is abnormally covered, but it will not do to credit the Chinese, on account of this, with having a superabundance of that material which is present in the brains of white men, because it has been flattened upon the side, pressing the supertemporal gyre over it.

What the President has said with regard to the unphilosophical —and I will go further and say unscientific—character of some of Benedikt's generalizations concerning the type of the brains of criminals, I approve of most heartily. It seems to me that Benedikt's statements merit the most careful scrutiny, because they are sensational in character, and I doubt whether all his statements will bear scrutiny.

With regard to the confluence of the central fissure with the Sylvian, our President stated that in but few does this occur, and while I should hesitate to say any thing on this point, except that the brains which I have examined confirm this statement, I may say that in between twenty and thirty brains I have not seen such confluence.

A good deal was said about the ape's brain, the extension of the parietal fissure, and the relation of the subject to *plis de passage*, which is perhaps true in fact, but I interpret them otherwise and should employ somewhat different terms; so I will reserve my criticism for a future time.

Finally, I should like to add a word with regard to the probability that a large number of human brains are more or less peculiar in some respect. It has been my fortune never to have had any but faulty brains, and I suspect that it is the case with a large number of those who have studied the brain carefully. But the majority of brains, which are studied carefully, I think are intrinsically more or less peculiar in some respect, which is noteworthy. As a singular illustration of the embarrassment to which a teacher may be put, I succeeded in obtaining half a dozen human brains for students to study, but when I came to examine them I found that I could not afford to allow them to dissect a single one, but I kept them all, because each showed some special striking peculiarity.

In conclusion : I should like to ask our President whether, supposing he had not known that the brain he was examining was that of a Chinaman, had not known but that it was the brain of an ordinary Caucasian, he would have felt disposed to regard the peculiarities which he has pointed out as any thing more than might exist in exceptional cases. In other words, is there any thing about the Chinaman's brain which would lead him to suppose that it belonged to a different race? For my own part, I should hesitate a good deal either to say their fissural type is unusually small, or that the relations of the fissures are peculiar in any marked degree. This is hardly a fair question to put, because the doctor knew that it was a Chinaman's brain, and I did also. However, I would like to see more Chinamen's brains.

There are, then, two points on which I wish specially to insist : first, that in two negro brains I have seen the same uncovering of the insula referred to ; and second, I have never but in one specimen seen fusion of the fissure of Rolando with the fissure of Sylvius.

Dr. F. X. DERCUM, of Philadelphia : I would like to make mention of one brain which I saw in the hospital for the insane, and which was taken from the body of an epileptic female. In this brain the fissures of Rolando and Sylvius were confluent on one side and nearly so on the other ; and the insula was covered on both sides. In some respects the brain was abnormal, while as to some other points it resembled the normal brain.

While listening to Prof. Wilder's paper, a communication made to the Philadelphia Neurological Society by Dr. A J. Parker was called to mind, in which he described several fissures on the mesial surface in children which afterwards disappear as the brain becomes adult. These fissures he described as being due to a mechanical cause, the brain growing more rapidly than the cranium does, and therefore is puckered up at various situations, and that these puckerings or creases unfold and disappear as the cavity of the skull enlarges. It seems to me that the peculiar relation of the fissures and the sutures would suggest also some mechanical relationship.

Mr. JOSEPH JASTROW, Ph.D., of Johns Hopkins University, was present, and was invited by the President to take part in the discussion. He thought it possible to make a picture of the human brain by the process of composite photography, which would locate certain convolutions as normal without producing such an

arbitrary map of the brain as had been given by Ecker, and he believed that the credit of discovering certain peculiarities belonging to the brains of criminals did not belong so much to Benedikt or to the German school, but rather to Lombroso of Italy. Their general conclusions are very much the same—that the brains of criminals do decidedly differ from the normal, but the differences are not sufficient to account for the types of crime so frequently manifested.

At present anatomy and physiology do not exactly fit each other, but perhaps with increasing knowledge of the functions of different parts of the brain these small differences, now regarded as unimportant, may be finally regarded as important, and then we need not look further than the brain to explain the differences in action between the normal sane man and the criminal.

Dr. W. R. BIRDSALL, of New York : The address of our President has brought up so many interesting points in connection with this vast subject, that one hardly knows what to leave out and what to mention. I simply wish to refer to a point with regard to Benedikt's conclusions. Some years ago, when his book first appeared, I had the opportunity to read it, and to write a review of it, and in that review I criticised his confluent-fissure theory as representing a type of the criminal class. It seems to me now that that has been the general opinion with most neurologists ; that is, that these views are hardly warranted by the facts. I believe, however, that the stimulation to study which Benedikt's book gave has been beneficial. The view, however, that we can look, to any great extent, for the origin of moral defects represented in criminals, to the variations in the convolutions of the brain, seems to me to be a narrow one. That certain classes of criminals do have brains which represent a peculiar and imperfect development, must be granted ; but that any particular type can be represented, the President has already opposed, and I agree with him in that particular.

I think, however, that the study of the cranium as to the different types of skull in different races, and their relations to fissures and convolutions, should be more thoroughly done than it has been. Of course, the immense number of brains which must be studied before definite results can be reached only shows how much must be done before reliable data can be reached.

Dr. B. SACHS, of New York : Greater importance must be attached to the development of primary than to secondary fissures ; the latter are controlled by mechanical influences.

The question suggests itself with reference to the brains presented by the President: Whether there was any peculiarity about the skulls in these cases.

The subject is undoubtedly a large one, but before we can decide with regard to mental conditions and their relations to fissures and convolutions, we should know first the dependence of these variations upon simple mechanical conditions.

Dr. WILDER: I think it is proper to say that the discussion should apply to primary fissures only. I do not think that we shall gain much in speaking of secondary fissures.

The PRESIDENT: With regard to Dr. Wilder's remarks, I would say that I fully agree with him that it would be better to harden brains within the skulls, and to prepare our specimens in that manner. But in many cases it would be impossible to apply this method. With reference to two of these specimens, they were obtained under *some* difficulties, and one or two of them under *great* difficulties.

I would say here that the fissure of Rolando, in one case, was crossed by a convolution instead of being confluent with another fissure. It is true, as he has said, that many brains are peculiar. This fact may be due to various causes. First, the majority of brains which are obtained for study are from persons of low mental capacity, and often of a low moral organization. I wish to impress the fact that the peculiarities in these brains are decided and unusual.

As to the Chinaman's brain, whether I would have been able to say that it belonged to a different race from the Anglo-Saxon? No! But my attention was directed to a peculiar appearance which seems to be different from that shown by the others, although I was unable to express it technically. The only special difference noticed first by Dr. Parker,—and I would be pleased to have Dr. Wilder examine the brain further and then tell us whether or not he believes that it is present—is an unusual obliquity of the orbital surface, which attracted attention.

It is wonderful how few brains of different races have been carefully studied.

With reference to Benedikt, while we should criticise him seriously, we should not be so absolutely severe as I have now and then known some to be. Many things which he says are facts, but I would say that he has a bad way of generalizing.

As to the brains of criminals, I believe that those of a certain class do give us a peculiar conformation and development.

As to standards of comparison, referred to by Dr. Gray, we need some one to carry on a work of that kind. It seems to me that the method mentioned by Mr. Jastrow would be interesting, nevertheless it would be questionable whether putting together a number of brains and getting a composite picture, would not be as tedious as the study of a number of brains and then getting a diagrammatic standard. Dr. Wilder has already answered Dr. Sachs' remark with regard to primary and secondary fissures. As to the skulls which held these brains I have a cast of that of Burk, but no observations were made with reference to the skulls in the other cases.

The Association then adjourned to meet at 3 P.M.

Wednesday, first day, afternoon session.

The Association was called to order at 3.10 P.M. by the President.

Dr. L. C. GRAY, of Brooklyn, read a paper entitled "A Case of Lesion of both Temporal Lobes Producing General Loss of Memory of Events without Word-deafness and without Deafness." (See JOURNAL,[1] vol. xii., p. 554.)

Remarks on Dr. Gray's Paper.

Dr. PHILIP ZENNER, of Cincinnati, O.: I would like to ask Dr. Gray to what extent the cortex was affected?

Dr. GRAY: Both temporal lobes were softened and the layer of gray matter was evidently much thinner than in other portions of the brain.

Dr. ZENNER: Was there any affection of speech?

Dr. GRAY: I am told that there was not the slightest; there was no lesion whatsoever of the faculty of speech or the perception of words; the patient's letters did not give evidence whatsoever of any disturbance of this kind.

Dr. ZENNER: I would not like to accept a case of this kind as one which refutes the ideas almost generally accepted, that one of the temporal convolutions is the seat of the reception of speech, if I may so speak; that is, that lesion of one of the temporal convolutions usually causes word-deafness.

The reason why I would not like to accept it as conclusive evidence is because there have now been so many favorable reports. So many cases of this kind have been reported in which the lesion has been situated here that it seems fairly established

[1] Refers to JOURNAL OF NERVOUS AND MENTAL DISEASE.

that it has a causal relation to word-deafness. But we cannot say exactly to what extent this convolution was destroyed in the case reported, and the meningitis does not necessarily destroy the functions of the cortex.

With regard to the temporal convolutions being the seat of hearing, that has been more or less definitely established, and that is one of the questions in physiology now quite generally accepted. But there is nothing, so far as I know, in pathology which would establish the view that the temporal convolutions are the seat of hearing.

Dr. V. P. GIBNEY, of New York, then read a paper on "Pseudo-hypertrophic Paralysis" (JOURNAL, vol. xiii., p. 257).

Remarks on Dr. Gibney's Paper.

Dr. GEO. W. JACOBY, of New York: Last winter I had the pleasure of administering to a case of pseudo-paralysis. At that time I looked up the literature of the subject quite carefully, and then I found in the *Glasgow Medical Journal*, for 1884, seventeen cases reported, and in none of those cases were there any changes in the spinal cord, so that the report in Dr. Gibney's case would be unique in that particular.

On the other hand, the muscles exhibited changes well known—fat in the muscular spaces, shortening of the muscular fibres, and increase of connective-tissue. But in the same patients there was no reference to the condition of the motor nerves of the muscles, except in one, and in that there was atrophy of the terminal filaments of the motor nerves. I would ask Dr. Amidon if he has examined the muscles in this case.

Dr. AMIDON answered that Dr. Gibney submitted a portion of a dorsal muscle for examination, but that he had not yet examined it.

Dr. SACHS, of New York: The subject of Dr. Gibney's paper is one of interest at this time, as diseases of the muscles are made the subject of very careful research.

In answer to some of the questions suggested, I wish to say that in contradistinction to the Charcot school we should take into consideration what Erb and his students have done in this field, and I would simply refer to some of the points to which he has called attention. I presume it is well known that in addition to the pseudo-hypertrophic paralysis which is common in the young, there is another form of muscular disease which has been

made out, and which is known as the juvenile form, as Erb calls it, of muscular atrophy ; and these two forms are classed under the term " progressive muscular dystrophy." He claims that we must distinguish between certain spinal-cord troubles and purely peripheral muscular troubles.

The ordinary form would belong to the purely muscular affection ; whereas the juvenile form of muscular atrophy would probably be classed as muscular atrophy due to spinal-cord affections.

There is a clinical point which would aid in making the differential diagnosis, and that is the electrical reactions in the case. In those cases where the peripheral muscles are affected only, the electrical reaction may be slightly diminished or increased, but not altered in the sense of the reaction of degeneration, as they would be in the cases of spinal-cord disease.

This entire question has an additional interest with special reference to another muscular disease known as Thomsen's disease.

There seems to be little or no doubt that all these diseases have certain points in common. It is a fact also that so-called peripheral muscular troubles exhibit changes in the muscles themselves. So far as I am able to make out, the changes are myositic in nature. Whether there be changes in the spinal cord also, remains to be seen.

Dr. ZENNER, of Cincinnati, O. : Of course, in the muscular nerves and the gray matter of the anterior horn, we have one single system, and it is important to know in what part the disease may be situated. Disease of the ganglion cells will produce secondary changes in the muscles and nerves, and we know now that disease of the muscles and the nerves will produce disease in the ganglion cells, and so it becomes important to determine which is primary.

It occurs to me that, in a case of this kind, where there is a great deal of external disease, disease of the muscles, and probably of the nerves also, that slight changes in the cord, changes apparently only in the size and perhaps the prolongations of the ganglion cells, and no changes otherwise, are probably secondary, and that this extensive peripheral disease is the more important factor.

Possibly, and of course, this is only a surmise, we can discover which is primary and which secondary, by noting where the most

extensive disease exists. In pseudo-hypertrophic paralysis, especially where no changes in the cord have been observed hitherto, it would appear that such slight changes would be the secondary and not the primary condition.

Dr. L. C. GRAY, of Brooklyn : It is easy to accept the views that peripheral lesions may cause central trouble, but usually when they do the symptoms are not purely motor. But there is no decisive proof that peripheral lesion is capable of producing central lesion, except where the suppurative neuritis is very marked and produces a very large peripheral lesion. For instance, in some cases neuritis has been set up in animals by caustic, which has set up a violent suppurative neuritis ; and so have some operations in the human subject, particularly some operations on the testicle and scrotum, and in one case an operation on a deep stricture.

Dr. F. X. DERCUM, of Philadelphia : There is probably no doubt that Dr. Zenner's views are most plausible, and certainly the life of the muscles is dependent upon motor cells, and if there are no more cells and the changes are such as we would expect, it seems to me that it is forced upon us as an inevitable conclusion. If a motor cell constitutes an important organ ; destroying one destroys the other.

Dr. ZENNER : I spoke of changes in the central organs as the result of mutilation of a leg or an arm, giving rise to extensive changes in the muscles and nerves.

The PRESIDENT : Did not Dr. Gibney ask a question as to whether pseudo-hypertrophic paralysis and progressive muscular atrophy are clinically distinct in the later stages ?

Dr. GIBNEY : Yes, sir ; that was one of the questions suggested.

The PRESIDENT : This question has not been discussed, or but very little. I would like to call Dr. Gibney's attention to the fact that one of the brains which I have presented to-day was removed from the body of a young man who exhibited both progressive muscular atrophy and pseudo-muscular hypertrophy. He was one of three brothers, in all of whom the symptoms of the two diseases were joined, and I had opportunity to see these cases from time to time for several years, and removed portions of muscles several times for microscopic examination. A paper on this subject was written and published by I. N. Kerlin and myself in the proceedings of the American Medical Association

some years ago—the year that it met in New York City. At that time there was discussion to some extent on the question of myositis and of the identity of the two affections, and my conviction was that they were the same disease practically, and that they were muscular rather than neurotic in character. Of the identity of the two diseases I am still convinced. In our cases portions of muscles were removed from the calves of the legs and from the back, and in the former the appearances were those of pseudo-hypertrophic paralysis, while in the latter they exhibited the appearances belonging to progressive muscular atrophy.

Dr. J. H. LLOYD, of Philadelphia : I reported to the Philadelphia Neurological Society several years ago the case of a girl who is yet under my care, who combines these two diseases in a most remarkable degree—pseudo-hypertrophic paralysis and progressive muscular atrophy. I have not sufficient details in memory to enable me to report the case fully here, and can only refer to the paper.

I wish to state, with regard to differential diagnosis between cord and muscle lesion, that it seems to me to be an exceedingly difficult thing to do clinically. In the case of the girl, she has great brawny calves, but distinct atrophy—of the forearm muscles especially, with fibillary contractions which are indicative of cord lesions. She also has slightly the reactions of degeneration, etc., and she seems to confute any effort at differentiating clinically between atrophy of the muscles, pseudo-hypertrophy, and true anterior poliomyelitis.

Dr. SACHS, of New York : This case illustrates what is well known, that two diseases occur combined in one person. For that reason it seems to me to be improper to speak of it as belonging to either. Why not speak of it as a case of progressive muscular dystrophy ?

Dr. J. H. LLOYD : I think the entire neuro-muscular apparatus is physiologically united, and that it is exceedingly difficult, if not impossible, to differentiate disease in one part from disease in another. There may be differences, but if you have fibrillary contractions and simple wasting of muscle, with a certain amount of electro-contractility intact, you probably have a cord lesion in which the entire apparatus is not involved, or wholly limited to the nerve centre ; whereas, with the brawny muscles of the calves we have a more decided muscular affection.

Dr. L. C. GRAY, of Brooklyn : I think that clinically there are

three affections. First, progressive muscular atrophy pure and simple ; second, pseudo-hypertrophic muscular paralysis pure and simple ; and third, a mixture of the clinical phenomena belonging to each of the others. It is very often a mistake to assume that a case is one of pseudo-hypertrophic muscular paralysis because there may have been some accident which has made the patient lead an inactive life, while at the same time the processes of digestion and assimilation may have gone on with their usual activity. I have a patient who has grown enormously fat under such circumstances, and the case, at first sight, might look like one of pseudo-hypertrophic muscular paralysis, which I doubt very much if it is. He has been, probably about ten years, almost helpless in his lower extremities and nearly so in the upper extremities, and at the same time has taken large quantities of food.

Dr. R. W. AMIDON, of New York : I would call attention to Dr. Zenner's remark that the lesion is too trifling to be one of the spinal cord, as compared with the changes outside of the cord. But in this case the proportion of ganglion cells is decidedly different from that seen in the normal cord ; the disproportion is very great indeed. The columns of Clarke seem to be about alike.

Dr. G. M. HAMMOND, of New York : I would like to ask Dr. Amidon if such destructive change had occurred, and if it began in the spinal cord, would it not almost necessarily have produced destruction of the contour of the cord ; that is, produced atrophy of the anterior horns themselves ?

Dr. AMIDON : I have thought that this case was very much more like those of progressive muscular atrophy than those of anterior poliomyelitis, and therefore there would not be any caving in of the cord, so to speak.

Dr. HAMMOND : A disease which begins in the cord and produces atrophy of the cells would be sufficient to produce caving in of the anterior horn ; whereas in these specimens the anterior horns seem to be in very good shape, the entire change apparently being in the cells.

Dr. DERCUM : I can hardly see that that point is well taken ; for I have the spinal cord of a case of progressive muscular atrophy in which there is, especially in the dorsal and cervical regions, absolutely no change in its contour, and yet there is marked lesion of the ganglion cells. In some of these cases there is rather an atrophic disease of the cells—simply a gradual disappearance of the cells.

Dr. GIBNEY: I have nothing further to add to the discussion, except to say that I am not sure that the lesion in the cord was sufficient to account for all the symptoms. I was in hopes that that question would have been settled definitely by Dr. Amidon and the other members.

Dr. Sarah J. McNutt, of New York, then read a paper entitled "On a Case of an Infant with Multiple Tumors of the Cerebrum, Probably of Specific Origin."

Syphilitic lesions of young children are rarely diagnosticated. They may occur as (1) generalized sclerosis, (2) miliary granules, and (3) as single gummatous nodules of large size.

In the adult the changes in the liver, the heart, the lungs, or the brain almost always assume the form of tubercles; their proggress is slow; they do not appear until a very advanced stage of the disease; and they belong as much by their date as by their character to the tertiary period.

The new-born, on the contrary, present them very early, often even as the first symptoms of the disease. The organs most frequently found affected in hereditary syphilis are the liver and the lungs, and the other organs more rarely present evidences of the disease.

According to Parrot the liver is most frequently found diseased in infants who die six weeks after birth, and it is upon the disease as presented in this organ, that the most reliable observations have been made.

Dr. McNutt reported a case, which was a marked one, of generalized gummatous infiltration; the liver and the spleen contained miliary gummata; in the lungs there was one nodule of the size of two peas; and in the brain there were multiple tumors, two being about the size of small marbles.

As in this case, syphilitic tumors rarely give rise to marked symptoms, and probably this explains why a diagnosis is so seldom made.

The autopsy was made by Dr. T. E. Satterthwaite, and the microscopic examination by Drs. W. H. Porter and W. A. Shufelt. The nodules were found to be composed of an abundant cellular formation interspersed with thickened blood-vessels and cheesy zones. A striking peculiarity of all the sections was a more or less thickening of the blood-vessels, with a marked tendency to a circular arrangement of the cell growth around the swollen and thickened vessels. Most of the cells were of the small round con-

nective-tissue type, with an occasional spindle-shaped corpuscle. Another striking feature of the growth was the fact that the cells filled all the intra-fibrillated and intra-vascular spaces.

Dr. G. Betton Massey, of Philadelphia, then read a paper "On the Cause of Electrotonus, and of the Normal Formula of Polar Reactions." (Journal, vol. xiii. p. 379.)

Thursday, second day, morning session.

The Association was called to order at 10.30 A.M., by the President.

Report of the Committee on Electrical Dosage.

Dr. Geo. W. Jacoby, of New York, read the report of the committee appointed at the last annual meeting.

"The Committee on Electrical Dosage reports that special rules applicable to all cases, for the use of the current strength, or for the length of time to be devoted to each application of electricity, do not exist, and can in the nature of things not be formulated. That all conclusions arrived at by any special investigator or series of investigators, no matter with what mathematical accuracy these conclusions be expressed, can only be arrived at empirically, and are dependent upon private experience and personal views. Therapeutic experience is too weak a foundation upon which to build an edifice of such magnitude as electrical dosage. Individual differences between patients and diseases would also render such a work of investigation futile.

"The committee, however, recommends the employment of all possible accuracy in concordance with present scientific knowledge in the application of electricity. It advocates the use of the measures adopted by the International Electrical Congress of 1881; also the use of an accurate galvanometer divided according to this system. Furthermore, the committee recommends the adoption of electrodes of certain diameters, with their square distinctly marked upon them in square centimeters (Erb's normal electrodes). This is necessary so that the current density may be known, for with a permanent current strength the density is inversely proportionate to the diameters of the electrodes, as expressed by the formula $D=\frac{I}{q.\ (\text{diameter.})}$ Knowledge of the current strength alone is not sufficient to enable us to gauge the efforts of the current upon the body, and therefore the proportion which

the current bears to its place of entrance and exit must be considered. If the current strength is represented by x, and the diameters of the electrodes by y and z, then the density of this current would be $\frac{x}{y \times z}$, or in figures, let current strength equal 6 milliampères, electrodes 6 x 12 cm., or 72 sq. cm., then D = $\frac{6}{72}$ or $\frac{1}{12}$, which means that upon 12 square centimetres of electrode 1 milliampère of current is spread.

"Therefore, it is also recommended that a system of expressing the current used, in concordance with these facts, be adopted, and that a fraction be always used of which the numerator represents the number of milliampères employed, and the denominator the number of square centimetres contained in the electrodes.

(Signed.) "Geo. W. Jacoby.
"W. R. Birdsall.
"R. W. Amidon."

The report was accepted and ordered to be printed in the transactions. It was then discussed by the Association.

Dr. L. C. Gray, of Brooklyn: I think, as this report receives the endorsement of the Association, that some other recommendations should be made; for example, as to what the covering of the electrode should be and what it is to be made of, as it will make a certain amount of difference in calculations, I think. For instance, an electrode of carbon will be much more conductile than an electrode made of sponge, and a metal electrode covered with absorbent cotton must be more conductile than one covered simply with sponge. So I think that these items would enter largely into the calculations, and that it would be well to incorporate something of that kind into the report to make a normal electrode still more useful.

Then I think it would be well to incorporate some suggestions as to what material for wetting the sponges should be employed, as it may make a good deal of difference, whether hot or cold water is used, etc. In using electricity myself I have been accustomed to have my electrical board over a sink which contains both hot and cold water, and my rule is to use water as hot as the patient can comfortably bear it. I would suggest that some such facts should be incorporated. And then I think it should be wisely incorporated as to the use of a galvanometer, and as to what kind of a galvanometer should be employed. There are in use a number of instruments, and there is a good deal of difference as to

which should be used. While we are making recommendations, I think that we should incorporate such little details as these.

The PRESIDENT : I would suggest that there is nothing in the resolution which would preclude the continuation of the committee another year.

Dr. J. H. LLOYD, of Philadelphia : I would like to say a few words on this subject. I have experienced a great deal of dissatisfaction in the use of electricity, especially in gynecological cases, because I do not know what strength of current will produce certain effects. We know that by covering the electrode with different substances a difference in the current strength can be produced, and this difficulty can be readily overcome. But the point to be known is, how much strength of current is required to do so and so ; say, to produce a catalytic action ; to produce, for instance, absorption of an inflammatory deposit in pelvic cellulitis ; or modify various neuralgias ; relieve dysmenorrhœa, etc. It is not what is to be the character of the cell to do these things, or the shape of the electrode, or its covering, but how much current strength do you require to produce therapeutic action. And this is where the Committee has not given us any information, yet it is where we require decided information. It is with reference to this subject that we have so much nonsense written with regard to electrolysis in the treatment of strictures of the urethra. If there is any such thing as electrolysis in strictures of the urethra we should know what current strength is required.

Dr. L. WEBER, of New York : So far as my experience goes with regard to electrodes, their size and kind, I have found that they may be oval, but that they should be, if possible, square. But it may be necessary to make some variations according to the part of the body under treatment. For instance, if the anode is applied to the thyroid, the electrode should be a band adapted to the size of the neck and head, giving a fixation surface, and should occupy a certain portion of the head. If I apply the cathode to the neck I use a long, square-cornered one, smaller or larger, according to the size of the patient.

The oval electrode will be useful, particularly, as far as the electrodes are concerned, when made out of charcoal. If we have the privilege of selecting, according to the statement referred to by Dr. Jacoby, and according to the results obtained by Erb's electrodes, we should have one as near square as possible, smaller or larger, according to circumstances.

As to the quantity of electricity to be used, that is as much an individual matter as any other remedy. There are no laws which can be laid down in this respect. It depends upon the individual and the amount of pain in which he receives the current. Central neurasthenia in one person will get along with two or three elements of ordinary size, while in another five or six or even more will be required. So it is impossible that any committee can be able to lay down any laws with regard to the quantity of electricity to be given. I think nothing better has been written in a long time than Müller's book, and he reccommends daily sittings of short duration and the use of very small currents.

Dr. G. BETTON MASSEY, of Philadelphia: I think that the committee has devoted too much time to the therapeutic aspect of the subject. It seems to me that the point to be gained is the determination of the number of milliampères required to move certain muscles, how many are required to produce a contraction, what the cathode closure is for certain muscles for comparison in cases of other muscles when diseased. I have recently made some experiments with regard to the number of milliampères required to move the extensor of the index finger for instance, taking a number of observations in order to compare them with normal muscles, with the muscles in writer's palsy, progressive atrophy of the hand, etc. It seems to me that a certain number of observations made upon normal muscles to determine the minimum number of milliampères required, would be of great value. According to my experience there is but little difference practically whether we use Erb's fine or Erb's small electrode, although the fine is doubtless preferable for muscles, as it limits the action to the muscles which we are experimenting upon. At the same time I do not wish to be understood as belittling the importance of the size of the electrode.

With regard to the remarks of Dr. Lloyd announcing the desirability of more accurate knowledge as to the measurement of electrolytic effects, I think that we have in the skin beneath the electrodes the nearest approach to the depth to which the current goes, or some idea with reference to electrolysis. If you apply an electrode upon a square inch of surface and send a current through it which shows a density on a galvanometer of ten milliampères, we have a density of ten to the square inch and we can note the electrolytic effects at any point. Suppose we wish to reach a tumor, how much of the electrolytic process can you

bring to bear upon it as compared with that in the skin? This can be determined by calculation. If, for illustration, the tumor has a central area of four hundred square inches and an electrode occupying one square inch of the skin upon the surface of the body is used, the density acting upon the square inch in the tumor is $\frac{1}{400}$ of what it is upon the skin at the surface. The only calculation required then is to calculate for the average spread of the current in the central plane, because the current will flow through these various lines (illustrated in a diagram), but it will be divided unequally between them; those lines which are most direct will carry more current than those which are indirect. But the difference is not so great as represented in works upon this subject; it depends simply upon the difference in length: if twice as long, it will carry twice as much current; consequently the density of the flow is not very great in a direct line, in a deep body, in a large conductor. The density of electrolytic influence to which a tumor or other body can be subjected by the constant current, through unbroken skin or mucous membrane, can be calculated by comparison with the plane of the body at which it lies.

Dr. JACOBY: I think that the primary idea in the appointment of this committee was to obtain a report on electrical dosage, and not to be a committee on galvanometers. If it had been a committee on galvanometers its work would have been less difficult; for it could have selected one of the many instruments and recommended it to the Association. The question of electrical dosage is a matter of personal experience, and such experience is not scientific, and is inaccurate. The mode of action of electricity does not enter into the duties of the committee; Ohm's law covers that.

The reason that no galvanometer was recommended was because the committeee did not wish to recommend the instrument made by any one maker. As to Dr. Massey's suggestion, that a standard galvanometer should be made, it would be difficult to find such an instrument; difficult to find a galvanometer that will remain exactly the same for a year. They will change, and are subject to magnetic influences, no matter how slight, which will lead to error in studying physiological problems; and for therapeutical use we need no such instruments.

As to the amount of current strength required to do a certain thing, if the committee were to inform the Association that a

certain number of milliampères will cure certain diseases, that again would be the result of individual experience simply ; and so far as electrolytic action is concerned the committee was not appointed for any such purpose.

As regards the question of electrical diagnosis, there again you will obtain contractions in different cases with a larger or smaller quantity of current. I do not think that any two cases of progressive muscular atrophy will give the same electrical reactions. Nor do I believe that absolutely normal muscles will give the same reactions to the same strength of current in different individuals.

The report of the committee was purposely that they were unable to do any thing on the question of electrical dosage ; they simply recommended accuracy in the application of electricity ; that is by milliampères and by regulated sizes of electrodes, and that when you do describe the amount of current it shall be in the use of these accurate terms. The question of current density will eventually lead up to it, as one of the scientific accurate methods to be pursued.

Dr. AMIDON, Secretary, then read a letter received by Prof. Dr. Aug. Forel, of Zürich, an associate member, and presented a photograph, which he had sent of a "Microcephalic Girl," fifteen years old, who had three microcephalic sisters who are dead. The father and the mother are normal.

Dr. WILDER'S paper was next in order, but before reading it he took occasion to make a few remarks retracting in part at least what he said concerning the Chinaman's brain, presented by the President at the first session of the Association.

I am prepared to say that it is the most remarkable human brain I have ever examined. And although we have no data which would enable us to say that it was the brain of a Chinaman —nothing in the shape except possibly a slight obliquity of the orbital surface, and which needs further comparison—nothing to point to its Mongolian character, it presents us with most extraordinary features, particularly the fact that the first temporal fissure extends not only as far as it usually does, but it actually sweeps up to the longitudinal fissure and into the mesal aspect of the cerebrum. This is distinctly a monkey feature. I do not think we are justified in making any derogatory comparisons, but it is distinctly one of the features which point to our ape ancestry. I have not seen this so well marked in any other human brain.

Dr. WILDER then read his scientific communication entitled "Notes on the Brain."

1. ADDITIONAL CASE OF INDEPENDENCE OF THE PAROCCIPITAL FISSURE.—In his recent paper on the paroccipital fissure, Prof. Wilder stated that among forty-three reliable specimens and figures accessible to him, the paroccipital is continuous with the

parietal in twenty-one and independent in twenty-two. The brain of a negro child at birth lately prepared by him had the two fissures wholly independent on the right and barely united by a shallow junction on the left.

2. AN ENTAL CORRELATIVE OF THE OCCIPITAL FISSURE IN AN EARLY FŒTUS.—In a fœtus estimated to be about six or seven months old, as shown in an accompanying photograph and figure,

opposite the occipital fissure there was a distinct ental ridge, so that the entire thickness of the wall was there folded. A similar ridge is figured by Tiedemann, but not named or described. Without further observations, it cannot be determined whether it disappears in the adult or persists as the more or less distinct elevation known as the *bulbus cornu posterioris* or *eminentia splenialis*. Even if transitory, it adds another to the list of "total fissures," including the calcarine, hippocampal, collateral, and Sylvian.

3. THE FŒTAL EXTENSION OF THE PROPLEXUS TO THE END OF THE POSTCORNU.—This is the case in a fœtus estimated to be five months old. The postcornual extension is apparently in process of atrophy. There were indications of a like extension into the precornu. These extensions might be expected in view of the great volume of the plexus at a still earlier stage.

4. POINTS ILLUSTRATED BY THE TRANSECTION OF A FŒTAL BRAIN.—Six points of morphological interest were noted, most of them indicating that the thalami increase in width as development proceeds, so that, in the adult human brain, they form, or seem to form, part of the floor of the procœles or lateral ventricles, which is not the case in other mammals, excepting, perhaps, the primates. (See also JOURNAL, p. 463, *et seq.*)

Remarks on Dr. Wilder's Communication.

Dr. L. C. GRAY, of Brooklyn: I was under the impression that some authors have made mention of the fact as to the thalamus not coming to the surface, and I was a little astonished to hear that such was the normal fact. I think it is alluded to in the recent work by Meynert and quite fully brought out.

Dr. WILDER: I will not deny that it is not so, but I have examined those works, as I have also almost all other works, and while it may be that it has somewhat obscurely been called attention to. I do not think that it has made sufficient impression upon anatomists to form a part of anatomical instruction. And I do not wish it to be supposed that I bring it forward as a new fact, because I spoke of it two years ago. But the point now is that I have seen it in the fœtus, and it has been claimed that no such thing occurs in the fœtus. Hadley and one or two others have called attention to the fact that the endyma is continuous over the surface of the thalamus, but nine out of ten teachers talk about the thalamus as a part of the floor of the lateral ventricle.

Dr. GRAY: I suppose it may be said that thoroughly accurate anatomical descriptions in text-books of the internal anatomy of the brain and basal ganglia do not exist.

Dr. WILDER: Quain may be taken as a type of the high-water mark of English anatomy. But I think, while it is amusing on the one hand and melancholic on the other, that he states the fact of the continuity of the endyma over the thalamus, the only figure in the book representing a trans-section of this part of the brain, leaves the student totally in the dark, and there is no attempt apparently to indicate that the endyma is continuous over the thalamus, but there is a vagueness which will not enable the student or teacher to make out what the figure is intended to illustrate.

The PRESIDENT: With reference to the nomenclature in connection with Dr. Wilder's first remarks on the "paroccipital" fissure, and also with reference to the physiological point coming out of the brains examined, it seems to me to be important to keep the developmental features in view in naming the various parts. Keeping such names as bridging convolutions and similar terms is of importance.

Then, with regard to the interparietal fissure, which Ecker represents as a single continuous fissure, as it is in apes and often in human beings.

Dr. WILDER: I must take issue there. It has not been seen in fœtal human brains.

The PRESIDENT: I think it has been so reported; the interparietal fissure passing back as a continuous fissure to what is commonly called the transverse occipital fissure, is a condition which I am sure I have often seen, but I do not say in the fœtus.

With regard to the difference between the left and the right side of the brain in a case of this kind, the view which I would take is that it can be explained upon a physiological and developmental basis.

Dr. WILDER: Supposing it to be true, does it not indicate a greater amount of cortex upon this side?

The PRESIDENT: Certainly it does.

Dr. WILDER: In other words, the fissure represents more cortex than the corresponding one of the other side.

The President presented the brain of a baboon, and also that of a new-born negro child, sent to him by Dr. Formad, of Philadelphia.

Dr. L. Weber, of New York, read a paper on "The Psycho-Neurotic Affections which Accompany and often Mask Phthisical Disease" (See JOURNAL, vol. xiii., p. 455, 1886).

Remarks on Dr. Weber's Paper.

Dr. L. C. GRAY, of Brooklyn: I think Dr. Weber's experience has been that of most of us who have seen many cases of tubercular nervous disease; in fact, the nervous symptoms preceding the detection by auscultation and percussion of pulmonary trouble.

I know that has been my experience in a large proportion of cases. In many cases also it has puzzled me so much that I have taken pains, and have sent my patients to gentlemen who have made a special study of auscultation, to see if they could detect any evidence of pulmonary disease.

Now, I suppose there is such a thing as tuberculous poisoning of the blood, the same as the syphilitic virus poison, long before it produces any organic changes, and long before it produces those changes in the small vessels of the pia mater which are particularly involved. I know of one case which is very interesting as an illustration of this fact, a case which I saw in consultation, of well-marked pulmonary tuberculosis. The individual began to manifest mental symptoms, and the question was with regard to the nature of the trouble. Death occurred, and the post-mortem examination was made by Dr. Leuf, whose reputation as a pathologist is well known, and who is competent to speak, and he found no changes whatsoever in the brain, although the person was delirious for four or five days before death. No microscopical examination of the walls of the blood-vessels was made.

With regard to treatment, I think that it is looked at in entirely too despondent a way. I have not seen a case cured, perhaps, but I have seen patients, for a time, have an absolute intermission of symptoms, produced by means of careful treatment. In a number of cases the symptoms have ceased and the patients have enjoyed excellent health, but in some cases they have relapsed.

I have found that the best treatment is some modification of that recommended by Weir Mitchell, of Philadelphia. Of course the patients cannot be placed in bed, but you can carry out the principles which underlie this treatment. The principle of absolute restriction of expenditure of energy to the greatest possible degree, and giving the largest quantity of food which the patient

can take, and that will be increased enormously if you restrict the expenditure of energy. If I can get these patients in the early stage, my prognosis is usually a good one.

Dr. WEBER : I am glad Dr. Gray has emphasized what I drew attention to in the therapeutic part of my paper, namely, the great importance of hygienic measures, particularly of the feeding of the patient. I would say in this connection that abroad, particularly in Germany, they are well aware of Mitchell's treatment of nervous disorders, and there are half a dozen institutions established already on that plan, and connected with large cities.

As to what can be accomplished by proper hygienic measures, by feeding or over-feeding in cases of tuberculosis, and by looking after the welfare of the patient, particularly in going into the minutest details, I have seen nowhere better examples than in a sanitarium near Frankfort. A similar institution has been established by Brehmer in Silesia, and most remarkable therapeutical results have been obtained.

Dr. JOHN VAN BIBBER, of Baltimore : I have had some experience in the treatment of melancholia, and I am accustomed to have the lungs examined most carefully. I fully agree with what Dr. Weber and Dr. Gray have said with regard to the desirability of appreciating properly the therapeutic measures that can be adopted in the treatment of melancholia, or the lung trouble which produces some mental derangement. And I should like to say here that I have used, with good results in such cases, the Sanguinis Bovinis Exsiccata prepared by Parke, Davis, & Co., as a concentrated food ; in some cases the results have been remarkable.

Dr. RALPH L. PARSONS, of Sing Sing : Having had charge of a number of insane patients, I have observed an intimate relation between melancholia and tuberculosis, to such an extent that for years, at regular intervals, I have carefully examined all the persons under my care for pulmonary difficulties. The frequency with which these patients will become phthisical without any thing except physical signs to indicate the existence of the pulmonary disease—even to the second and third stages—is remarkable.

Dr. PHILIP ZENNER, of Cincinnati, then reported a case of " Auctioneer's Cramp," and presented the patient.

This was a professional neurosis, but differed from others in affecting some of the muscles of articulation. No similar case had been reported, so far as the speaker knew. He termed the

disease "auctioneer's cramp," on account of the occupation of the patient. The latter was forty-five years of age, and had always been in good health previous to his present trouble. He had been an auctioneer for many years, and applied himself with unusual diligence to his business. In March, 1885, he was confined to his house for a month with an ulcer of the leg, but during this time his general health was good. At the first sale he cried after returning to his business he observed a slight but scarcely noticeable difficulty of articulation. This occurred only when a word—as five—was frequently and rapidly repeated, as often occurs in crying a sale. This difficulty rapidly increased in succeeding sales, so that after crying four or five more he was compelled to discontinue them altogether. He now began to observe that something of the same character of disturbance would occasionally occur in ordinary conversation, and he would have a sense of painful fatigue about the left corner of the mouth, which soon became an almost continuous sense of discomfort. The speaker first saw the patient about a month after the beginning of his trouble. He was under his observation then for about a week, and has been again the last few months. His condition during that time was about as it is at present, though the symptoms seem more marked at the present than any previous time.

The patient was then presented to the Association, and his condition demonstrated. In a state of repose there is nothing noticeable excepting that the naso-labial furrow on the left side is a little deeper than on the right, and the left angle of the mouth seems a little drawn in, as if pressing against the teeth—a condition due to a slight tonic contraction of the muscles. Usually there is nothing special observable in ordinary conversation, so that many of his acquaintances have never noticed any thing peculiar about him. But occasionally, when he speaks, a contraction of the muscles on the left side of the mouth is noticeable. The special symptoms are brought out by rapid speech. When he repeats the word five rapidly, as in crying a sale, a state of spasm of some of the muscles ensues, which soon makes further speech almost impossible, though the spasm ceases when the effort is discontinued. During this effort the muscles of the left corner of the mouth, chiefly the orbicularis oris, contract very forcibly, pressing it firmly against the teeth, and at the same time the muscles on the right side draw the right corner of the mouth upward and to the right. The chief spasm is in the muscles at the left

corner of the mouth. This part feels, to the examiner, as hard as a board, and at this place the patient has a feeling of very painful fatigue. The upper lip and part of the face takes no part in the spasm, and the upper lip and cheek on the left side of face are flaccid, while the right corner of the mouth is drawn upward and outward, that it appears as though the left side of the face were paralyzed. But this is easily disproved when the state of spasm has passed away. The patient can then whistle, expose the upper or lower teeth, as well on the left side as on the right, or make any movements of this kind forcibly when resistance is made by the examiner.

The patient can prevent these spasms by a very simple measure, lifting the left corner of the mouth during the act of speaking. When he does this he can repeat the word five rapidly without any facial spasm ensuing. The speaker thought this act prevented spasm through keeping the orbicularis oris, the muscle chiefly affected, in a state of extreme extension.

During the facial spasm, and for a short time afterward, the patient has a very painful sensation, a sensation like that of fatigue, especially at the left corner of the mouth, but extending beyond this. But a certain sense of discomfort is more or less constantly present on the left side of the face. Also after the facial spasm passes off, one may observe at times slight contractions of the muscles about the left eye and cheek, contractions almost fibrillary in character.

The patient suffers a good deal with mental depression, general nervousness, etc., but further than this there is no sign of disease. The fundi oculi and patellar tendon reflexes are normal; no paralysis of sensation or motion can be detected anywhere, etc.

Only the orbicularis oris was carefully tested with electricity, as this was the chief muscle affected with the spasm. The muscular contractions were sharp and quick like those of normal muscles, and were produced when the electrode was applied to the muscle or to its nerve. There seemed to be lightened faradic contractility on the left side, and heightened sensibility to the current. Tested with the galvanic current, the anode closure contraction was produced as quickly as the kathode closure contraction; otherwise the reaction appeared as normally.

The speaker had classed this case among the professional neuroses, because it has the distinguishing features of that group of diseases, its chief or only cause being the excessive use of the af-

fected muscles, and the special symptoms being evoked only when those muscles are called into play by a voluntary act. The patient believes there was some further cause than excessive use of muscles, catching cold, or the like. But no other cause has been found.

The speaker believed in this case the cause was the excessive use of a few muscles. The crying of a sale consists largely of the frequent and rapid repetition of certain words, as five, five, five, etc., etc., which keep in almost constant activity a few muscles, while there is not a continual change in muscular activity as in ordinary conversation.

The treatment has consisted chiefly of the use of the galvanic current and internal administration of arsenic and iodide of potash. The patient has discontinued his crying of sales, but it seems as if mere conversation kept the affected muscles in sufficient activity to maintain the constant sense of discomfort in the face, and to prevent a cure. If other measures will not avail, the speaker proposed the stretching of the left facial nerve.

Remarks on Dr. Zenner's Case.

The PRESIDENT : This case is a highly interesting one, and we are greatly indebted to Dr. Zenner for having brought it before the Association. I am inclined to the view that it is a real professional neurosis, although it opens up several points for discussion.

I might relate a case, which I think has not been published, similar in character. The gentleman was one of Dr. Charles S. Turnbull's patients. He was an actor and was compelled to put his face into peculiar positions, and in consequence of keeping his face in these forced and distorted positions—that is, we concluded it was in consequence of this—he came to Dr. Turnbull with paresis of the external rectus and distinct double vision. There was this peculiarity about it, namely, that the man, while vision was distinctly double, could with increased voluntary effort pull the eyeball around—he could throw power into the affected nerve muscle. This shows the truth of the view that some of the ocular movements are due to true central control of a peculiar kind. That patient has almost entirely recovered, although his difficulty lasted for some time. The case presented by Dr. Zenner suggests several things which are similar to what was seen in Dr. Turnbull's case.

The secondary contractures of Hitzig are well known—in facial paresis we have the occurrence of secondary contractures, or a combination of tonic and clonic spasmodic movements, which are apt to be continuous and difficult to alleviate. The appearance of this man's face is precisely like contracture which I have seen after facial paralysis.

It is not impossible that there exists a slight peripheral neuritis, and it may have been the starting-point of the special phenomena now present, but the character of the spasmodic movement has been implanted by the habit to which these muscles have been subjected. It shows that it is possible that there are cases of central overwork, volitional overwork, but such cases are more likely to occur amongst those of other employment than auctioneering, where the movements are almost automatic.

I believe that the line of treatment suggested by Dr. Zenner is a very good one. Certainly the indications would be to give complete rest to the centres, to restore the nutrition of the parts affected, and to diminish the mental irritability as far as possible. The last suggestion should be resorted to, namely, that of stretching the facial nerve, but all other measures should be used first. Static electricity and galvanism would probably be beneficial, but faradism would be likely to do harm, or at least would not be so likely to do good. The cautery to the face or neck might also be used.

Dr. L. C. GRAY, of Brooklyn : I think the diagnosis in this case lies between a cortical lesion, polio-encephalitis, and lesion of the facial nerve. Of course the differential diagnosis would have to be made by more careful electrical examinations than have been made, because there might be some slight difference between the two sides with the galvanic current and some possible alteration in the progression of the poles which would throw some light upon the subject. I think that the man is paralyzed on one side of the face. I do not think he closes his eye as firmly upon one side as upon the other, and when the eyes are open there is a distinct paralysis of the lower portion of the face. When he closes his jaw he overcomes the resistance upon the right side, but he had to make quite a distinct difference upon the left side. There is a distinct difference in the motor power upon the two sides.

The case differs from the ordinary forms of writer's cramp at all events. I should recommend the trial of the galvanic current, trying the positive pole. In the treatment of the superficial

muscles of the face I believe in the special efficacy of the positive and the negative poles. I would also recommend the use of the instrument of Adamkiewicz, and the application of an anæsthetic through which the current is passed, and by which you carry the liquid into the tissues. There has been much dispute as to whether that can be actually done ; but I know that it can be done, as I have obtained complete anæsthesia with a ten-per-cent. solution of cocaine in the parts surrounding the poles.

Dr. L. WEBER, of New York : In looking at this case and examining it, many points must be taken before the examination is complete. But certainly the case does not impress the impartial observer as one which began as a neurosis, because it is unilateral, the muscular movements are not tonic, but rather clonic, and are more secondary, as are often seen in post-hemiplegic affections. The activity of the entire right side of the face is undeniable. A positive paralytic condition is present on the left side, and whatever there is interesting as a case of neurosis, there must have been a certain amount of neuritis with regard to the left facial. Even the patient has the recollection that he felt a lameness on the left side of the face before these peculiar spasmodic contractions of certain muscles occurred.

Therapeutically, be it a combination of a neurosis and a neuritis or not, the galvanic current is indicated, used in such way, perhaps, as Dr. Gray has stated.

Dr. ZENNER, in closing the discussion, remarked that he would not deny that there was a neuritis present, but there had been no evidence of it—no paresis, and no reaction of degeneration. Whether a neuritis could be detected or not, would not affect our considering the case a professional neurosis, for such a classification is based chiefly upon the cause of the symptoms. The true nature of the professional neuroses, at least in most instances, is obscure, just as it is in this case. In some instances of writer's cramp a neuritis has appeared to be the cause, but the diagnosis of such a condition does not necessarily remove it from the group of professional neuroses. Possibly such a condition is the basis of them all. The apparent paralysis of the left side of the face, during the facial spasm, is not real. That the muscles retain their power is easily shown when the patient is in a state of calm, but not so easily shown in his excited state when before so many physicians, though it was then shown how he could expose his teeth equally on all sides, etc. The speaker had examined him very

carefully for paresis of the facial muscles, especially the first time he saw him, but never detected any.

The Secretary then read the following translation of a letter from Prof. Gudden, made for him by Dr. Jacoby.

MUNICH, July 15, 1885.

Highly Respected Colleague :

Herewith acknowledging the receipt of your letter of the 30th of June, I beg of you to transmit my best thanks to your respected Society for the honor which they have done me in electing me to their corresponding membership.

With highest respects,

GUDDEN.

The Association then adjourned to meet at 3 P. M.

Second day, afternoon session.

The Association was called to order by the President.

Dr. BURT G. WILDER, of Ithaca, exhibited a living frog which was decerebrized more than seven months ago.

The animal had enjoyed perfect health ever since the operation was performed, which was on the 9th of December, 1885. He had been fed on small fish or pieces of meat twice a week, but two persons were required to feed him, as he would not open his mouth voluntarily. Just here a curious phenomenon presented itself ; that is, the frog did not know any better than to attempt to do two things at the same time, which were the reverse of each other in design. If a piece of a minnow's tail projects from his mouth, he would make an effort with one or both of his fore feet to remove the fish from his mouth, while at the same time he endeavored to swallow the other end. He would change his position slightly, would balance himself, wink with one eye, make the retrograde movement, and when irritated would wink with both eyes. Dr. Wilder queried whether the frog ever slept, whether he could be hypnotized, etc. It was a question also whether he was capable of any kind of education. He would lie upon his back, although not hypnotized. It would also be interesting to know whether or not a pair of such frogs could procreate, or whether this frog was capable of procreating.

Dr. SACHS, of New York : With regard to some of the questions suggested by Dr. Wilder, there is one essential difference between the manner in which he has decerebrized his frog and the manner

in which the operation is done in Goltz's laboratory. There, nothing is removed ; the parts are simply severed subcutaneously. Dr. Wilder's method is much more thorough, and I am surprised that the frog has lived so long. There is one point which corroborates what Goltz has found, and that is with reference to laying the frog upon the back ; he noticed that, and also that if the legs be drawn out slowly they will be retained in any position, however uncomfortable that may be. This it is also possible to accomplish in functionally decerebrized animals of much higher order than the frog.

Dr. JASTROW, of the Johns Hopkins University : I can answer one question suggested by Prof. Wilder, and that is with reference to hypnotism, or rather catalepsy. I have a series of frogs which were operated upon in the following manner : First, normal ; second, frogs in the condition of Dr. Wilder's ; third, with the optic lobes also removed ; and fourth, the ordinary reflex frog, with every thing removed above medulla. The very last thing that a frog gives up, is lying upon the back. None of these frogs, except the reflex frog, will, under ordinary conditions, submit to being laid upon the back.

Apparently this action, whether it is an hypnotic influence or not, will be left out of account. At all events this cataleptic condition remains as long as any thing of the brain is left at all. Furthermore, these frogs can be etherized.

Dr. WILDER : Did Dr. Jastrow succeed in cutting across the brain between the cerebellum and optic lobes ?

Dr. JASTROW : Of course you cannot always be certain that this condition has been obtained.

Dr. WILDER : Has the condition of the brain, in such experiments, been observed to see whether there is any repair or reproduction ? has there been any indication of reunion after transsection made subcutaneously ?

Dr. JASTROW : I know of nothing with regard to the frog, but with regard to dogs it has been carefully studied, and no such union has been observed.

Dr. GRAY suggested the acetic-acid experiment, and Dr. WILDER said that it had been applied with the usual result.

Dr. JASTROW : I accidentally found a frog with one leg bitten off, and when the acid was applied to his back, he not only attempted to brush it off with the sound leg, but when that was held, he made the same attempt with the stump of the other leg.

Dr. C. L. DANA, of New York, then read a paper on "Pseudo-Tabes from Arsenical Poisoning."

The object of his paper was to report two somewhat unique cases of arsenical paralysis, presenting the symptoms of tabes dorsalis; next, to show that arsenical paralyses, like those from diphtheritic poisoning and alcohol, present two types, one of which might be called the mixed or ordinary form, and the other the ataxic form; and to show also, if possible, that the ordinary teaching that arsenical paralysis is due to a diffuse myelitis is not correct, and that these paralyses are really the result of a multiple neuritis. The conclusions arrived at were: 1. That a disease resembling locomotor ataxia may be caused by arsenic given medicinally, absorbed from wall papers, or taken in a single large dose. 2. That arsenical paralysis of this type and arsenical paralyses of other types are not due to a diffuse myelitis, as has been taught, but to a multiple neuritis. 3. That arsenical paralyses, like those from diphtheria, alcohol, and probably other infections and poisons, are of two types: *a*. The ordinary mixed, motor, and sensory paralysis; *b*. the pseudo-tabetic form, in which there is no pronounced motor paralysis, but marked sensory troubles, especially ataxia.

Remarks on Dr. Dana's Paper.

The PRESIDENT: The subject of arsenical paralysis is one in which I have been practically interested. Dr. Dana has had the kindness to refer to a paper which I wrote several years ago, which was based upon careful examination of one case that I saw in consultation, and afterwards, through the kindness of Dr. Mitchell, had opportunity to study it for months until finally the patient recovered; and also the study of the history of other cases which were affected at the same time. Since that time I have had other cases of arsenical paralysis. One of these cases I reported in a series of lectures delivered on the differential diagnosis of myelitis.

In the paper referred to by Dr. Dana I expressed the opinion that the evidence was in favor of diffused myelitis, but I also stated there what I wish to restate here, and enlarge upon, namely this point, that in cases of so-called arsenical poisoning, in the cases which I have seen, not only the nervous system has been affected, but other parts of the body have also been affected, as in cases of poisoning by mercury, or through the infectious diseases,

such as diphtheria, etc.; and that the true light in which to look upon the subject is not to regard these cases as either instances of myelitis or neuritis or myositis, but, taking the idea expressed first by Ringer, to consider that they are cases of protoplasmic poisoning, affecting not only the nervous protoplasm, but the tissues of other organs in other parts of the body. Therefore, while Dr. Dana asserts his reasons for regarding the disease as a neuritis, I wish to recede from the ground, if I ever took it, that it is solely a myelitis. This is true of a great many of these diseases. Just now we are in an era of multiple neuritis, and able investigators have proved sufficient to show that there is truth in these views. Take a case of alcoholic paralysis. Dr. Sinkler will recall a case which was also seen by Dr. Reed, and which was variously diagnosticated by different physicians, all men competent to reach a correct diagnosis. This man, however, after having all the spinal symptoms of arsenical poisoning, after a time developed cerebral symptoms, and then it was called a case of paretic dementia; he probably developed an encephalitis. Take the cases referred to in my paper on arsenical paralysis. One died with all the symptoms of diffused encephalitis; one or two were nearly permanently paralyzed with chiefly spinal symptoms; others had more or less symptoms indicating neuritis. Now what we wish is more satisfactory general considerations. Dr. H. C. Wood holds largely to the doctrine of neuritis, in connection with cases of this kind. If you come to a strict analysis of these cases the evidence of neuritis depends upon one or two things, especially pressure upon nerve-trunks. My own experience is that we have a diffused hyperæsthesia in many cases not neuritic, and this may be mistaken for nerve pain. A hyperæsthetic condition is undoubtedly present in subacute myelitis of the anterior horns, of Duchenne. I simply throw out this point for discussion.

Dr. GEO. W. JACOBY, of New York: I think that if we should look for similar cases as the result of poisoning they would be found. I once wrote a paper on lead-poisoning, and I reported in it a case of tabes due to poisoning by lead, and it was almost impossible to make a differential diagnosis between it and true tabes. The only astonishing part was that the man had paresis of the extensor muscles of the middle fingers of each hand, and ultimately the case was diagnosticated as pseudo-tabes due to lead-poisoning. I then expressed the opinion that I was more inclined

to the view of ataxy being due to lesion in the cord, if lesion it might be called, but I would now report it as a case of neuritis, due to lead-poisoning, and I think that Dr. Dana's ideas, in the main, are correct on that point. I would also add that the patient is absolutely well now, with the return of the patellar reflex.

Dr. B. SACHS, of New York : I rise to enforce one point : We speak of these cases as arsenic and lead cases, apparently implying that they are all very much alike. That would seem to imply that the pathological lesion would be similar in all these cases. The analogy has been referred to between these cases of arsenical and lead-poisoning on the one hand, and on the other the poisoning of diphtheria and other infectious diseases. We know with regard to diphtheria that its poison may attack different parts of the body. Now it seems to me unreasonable that the effect of arsenical and lead-poisoning should always be exhibited at one and the same place in the body, or in any one organ only. I do not see why the infection may not affect the peripheral nerves in one case and the spinal cord in another. A large number of investigators have found in post-mortem examinations distinct lesions of the anterior horns of the spinal cord in lead-poisoning, and an equal number of other reliable observers have found lesions of the peripheral nerves. I cannot see why arsenical poisoning may not be productive of myelitic changes in one case, and of neuritis in another. The clinical symptoms will be very much alike, and cases of one kind have been mistaken for those of the other very many times. I would furthermore call attention to the fact that in cases of genuine tabes changes have been found *post mortem* in peripheral nerves. Recent experiments would lead us to suspect that many peripheral symptoms of tabes, such as pain, are possibly not due to change in the spinal cord, but rather to changes in the peripheral nerves. I would ask Dr. Mills for his opinion with regard to the toxic agent affecting different organs and parts of the body.

The PRESIDENT : I would say that the position which I hold practically is that which Dr. Sachs has just expressed. I am not attacking the view of Dr. Dana. We have neuritis in some of these cases, perhaps in the majority ; but I wish to take the position that arsenical poisoning, or poisoning from lead or mercury, or from infectious diseases, may give us neuritis, instead of myelitis or encephalitis by preference perhaps, but that they may give us one or all of these affections.

Dr. DANA: I perhaps put my cases rather more strongly than I felt myself, for the reason that the ordinary teachings are that arsenical paralysis is due to diffuse myelitis, so far as I could find them.

When I first saw these cases of arsenical poisoning I made up my mind that they were cases of neuritis. I then looked up the literature of the subject and found that Dr. Seguin and Dr. Mills and others did not believe that they were cases of neuritis. Therefore I put my cases rather strongly, perhaps. I believe that the main thing to be kept in mind is the fact that there is a form of tabes, resembling locomotor ataxy, which may be caused by arsenic, and that it may be mistaken for locomotor ataxy. Consequently I wish to bring out the fact that, so far as evidence goes, in the cases of arsenical poisoning these symptoms are mostly due to multiple neuritis. I did not say that all cases were due to neuritis; I do not suppose that that has been proved; but I think that the views expressed by our distinguished President and others, that mineral poisons sometimes affect the central and sometimes the peripheral parts, are plausible, but it seems to me that in most cases it is a peripheral neuritis.

Dr. Wharton Sinkler, of Philadelphia, then read a paper on "The Treatment of Facial Spasm." (See *Medical News*, Sept. 24, 1886.)

Remarks on Dr. Sinkler's Paper.

The PRESIDENT: Dr. Keen sent this patient to me within two or three weeks, and I found that the woman was doing very well.

First, a word with regard to the method Dr. Keen adopted to determine absolutely when the nerve was obtained; it certainly is a very practical point. I resorted to this method six or seven years ago in the Hospital of the University of Pennsylvania. We had then a case of neuralgia affecting the hand, and we determined to stretch the musculo-spiral nerve. The operation was performed by Dr. Ashhurst, and I suggested that a weak current be applied as we proceeded with the operation, to determine what nerve was being stretched. In another case which I recall, something which was not the nerve at all was resected, and a second operation was performed and the nerve stretched. This mistake might have been avoided if the battery had been used.

Dr. C. L. DANA, of New York: With regard to the statistics

offered in the paper, it seems to me that too great value is attached to them, for statistics are extremely fallacious. We have with this form of neuralgia, as with others, different causes and different peculiarities in different cases, and perhaps some of the cases would have gotten well under a slight operation of any kind, and other cases would not have improved under any treatment whatever.

As the statistics were put, it would seem that five out of twenty-one were likely to get well, but when analyzed it would seem that one or two out of the twenty-one got well. The case reported has been operated on only *four* months, and it is possible, or even probable, that the woman will return within a year.

Dr. E. D. Fisher, of New York, then read a paper entitled "Remarks on Epilepsy" (see JOURNAL, vol. xiii., p. 481, 1886).

Dr. B. SACHS, of New York, read a paper on "Intracerebral Hemorrhage in the Young." (To be published in this journal.)

After referring to the increased attention neurologists were paying to the cerebral accidents of children, the writer recorded his conviction that intracerebral hemorrhage is more frequent in children than is generally supposed, and that many cases of this sort are classified under the head of meningeal hemorrhage, Dr. Sachs then reported two cases of intracerebral hemorrhage : one in a boy two and a half years of age, and the other in a young man of nineteen years. The first case was given in full, in order to place the diagnosis upon a firm basis. The child had typical right hemiplegia with aphasia, without coma convulsions at the time of onset. The onset was slow, aphasia setting in first, paralysis of the arm and leg some hours later. The recovery was typical of that which takes place in many cases of adult hemiplegia from apoplexy.

The writer gave the reasons why he held that in this case the apoplectic attack was due to hemorrhage rather than to embolism or thrombosis. As regards the differential diagnosis between meningeal and intracerebral hemorrhage, the lack of convulsions seems to be of unusual significance. In meningeal hemorrhage convulsions are invariably present, and their absence might argue, other things being equal, in favor of intracerebral hemorrhage. In the second case the young man had had two apoplectic attacks exactly one year apart. The histories of these attacks were very similar to the one given in the first case. In the first mild attack all symptoms developed and receded typically; in the

second attack the onset was slow, there was coma lasting for over eighteen hours, and recovery is not yet complete, contraction having set in in the affected arm. No convulsions at any time. There was no specific history.

Using these two cases as a basis, the author of the paper entered upon a discussion of the changes in the walls of the cerebral arteries, permitting an effusion of blood into the brain substance. Autopsies on this condition are very scarce, but there is good reason for supposing (reference was made to some recently reported cases of Dr. Osler) that miliary aneurisms occur in young children, and that fatty degeneration of the cerebral arteries, permitting transudation of blood through the vessel-walls (Recklinghausen), is a not infrequent condition.

Discussion was invited on the following points :

1. Frequency of intracerebral as compared with meningeal hemorrhage in young persons not the subject of specific disease.

2. The value of convulsions as a factor in differential diagnosis.

3. Pathological changes in the walls of the cerebral arteries in the young.

Remarks on Dr. Sachs' Paper.

The PRESIDENT : As to the occurrence of intracerebral hemorrhage in the young, I think it is not of such infrequent occurrence, although there are but few reports of autopsies. I have made autopsies in three cases. The first was in a very young child. Three children of the same parents had, in succession, died almost immediately after birth. I made an autopsy in the third case, and found extensive hemorrhage. In another case I found meningeal hemorrhage, much diffused. This case presented a peculiar condition, which was of interest physiologically, and a point worth looking for in very young children, and possibly may have some diagnostic significance. In ordinary hemiplegia we have paralysis of the arm and leg and slightly of one side of the face ; in other words, the muscles of the trunk and abdomen are not very much affected. In young children the hemorrhage is likely to result in paralysis which involves the muscles of the chest and abdomen, and especially the face.

Dr. Lloyd will perhaps give us a word about a case which he related to me yesterday, although it was in an adult.

Dr. J. H. LLOYD, of Philadelphia : At the University Hospital Dispensary, some years ago, I saw a young man who had distinct right hemiplegia with aphasia, following diphtheria. I was not

the physician in attendance, and did not see him until the hemiplegic condition had intervened. I had reason to doubt its being an ordinary diphtheritic paralysis. Probability was that this case was one of embolism causing hemiplegia with aphasia.

The other case to which the President has referred, was that of a young man eighteen years of age, to whom I was called and heard the statement that he had been sick all day, had sat down upon the sofa, and had rapidly passed into the condition of coma with convulsions, more especially on the right side. After the boy's death I got a history of former alcoholism. He lay in this comatose condition about twelve hours, and, so far as I could judge, with this comatose condition he had right hemiplegia. In addition, he had sweeping over the paralyzed side, waves of clonic spasm. Besides, he had extreme right lateral deviation of the eyes, and also high temperature. He also had the appearance of right facial paralysis ; the face was distinctly drawn to the left side.

I was a littled puzzled in making a diagnosis. I was unable to find out any thing with reference to history of specific disease. I concluded that he had meningeal hemorrhage. Dr. Formad made the post-mortem examination, and nothing could be found to account for the bilateral convulsions. There was an œdematous condition of the membranes, and a very decided congestion of the entire brain ; an ecchymotic condition of the skull, especially over the motor regions, as though bruised, and presenting a bluish color, as though saturated with venous blood. He also had decided venous congestion of the kidneys. There was no hemorrhage anywhere in the brain. I finally regarded the case as one of acute exacerbation of alcoholism engrafted upon a chronic alcholic condition probably favored or complicated by uræmia.

Dr. ZENNER, of Cincinnati : I would like to say, with reference to the first case, that it seems to me to be of far greater importance to recognize the frequency or the infrequency of such cases than the nerve symptomatology presented.

In cases of central hemorrhage, embolism, and thrombosis, usually no positive diagnosis can be made from the mere symptomatology. The mere fact of paralysis coming on slowly would not be sufficient to make a diagnosis, because embolism might occur so that only a branch of an artery would be plugged up at a time. The mere fact of the absence of any definite cardiac symptoms

would not be sufficient to exclude such a diagnosis, because slight changes in the heart are not revealed by physical signs. Furthermore, the heart is not the only source of emboli. Therefore, the mere symptomatology alone will scarcely enable us to determine whether we have a case of embolism, or thrombosis, or hemorrhage before us. When we take this fact into consideration, and the fact that hemorrhage in the young is so very rare—a fact not based upon speculation, but upon examination of a large number of cases, perhaps it may be found more frequently than has yet been determined,—taking these facts into consideration, it would be scarcely wise to accept cases like these as subverting previous views concerning the great rarity of this disease, and it would look like embolism rather than hemorrhage, because of the rarity of hemorrhage in the young. At present the probability is in favor of the former.

Dr. SARAH J. MCNUTT, of New York: I have not seen a case of intracerebral hemorrhage in young children. I will mention a case of paralysis on one side preceded by convulsive movements of the same side of the face, and very marked in the eye. The child died, and I expected to find some lesion in the brain, but nothing except marked œdema in all parts was found.

Dr. R. W. AMIDON, of New York: This paper has interested me very much. It seems to me that the lesion must be looked for in the circulatory apparatus, and more likely in the arteries. The most natural lesion would be, perhaps, the result of periarteritis, and then miliary aneurisms referred to, but according to my experience and reading, I should be inclined to think that periarteritis resulting in miliary aneurisms is a disease of adult life, and usually of advanced adult life. Of course I except the periarteritis which occurs in tuberculous children. Endarteritis from syphilis in children is rare, and also from any other cause. Therefore, I should throw out the supposition that miliary aneurism could cause these hemorrhages. But there is one cause of cerebral hemorrhage in young persons, to which none of the speakers have called attention, and that is the condition which has been well described by Barrie in the *Revue de Médecine*, in several articles, which are summed up in the July number—namely, congenital narrowing of the aorta. I have seen pathological specimens taken from the bodies of two boys, sixteen and nineteen years old, who had congenitally small aortas (N. Y. Path. Soc.), and both of whom died of intracerebral hemorrhage, and another case has

been reported to the same society, but I did not see the specimen. Barrie reports twenty-seven cases of narrowing of the calibre of the aorta, and he says that it is almost always below the origin of the left subclavian artery, which would give a stronger reflux into the left carotid than into the right. He says that this construction or narrowing is of three forms: (1) funnel-shaped; (2) cul-de-sac formation; and (3) interposition at certain points of an almost complete diaphragm with a small circular or lateral opening. In his cases, some of which never gave rise to symptoms, the patients having died of something else, of the pathological appearances found at autopsy, cerebral hemorrhage, while not the most common, was observed occasionally, and that the two conditions stood to each other in the relation of cause and effect is very plain indeed. Collateral circulation may compensate somewhat, but it can never take the place of a normal circulation.

Dr. SACHS: With regard to the interesting paper by Dr. Barrie, referred to by Dr. Amidon, it is especially interesting in one respect. No doubt that this is a frequent primary cause, yet I question very much, and in this Dr. Amidon will probably concur with me, whether that condition of the aorta would have caused cerebral hemorrhage if the arteries in the brain had not been previously diseased.

Dr. AMIDON: In most of the cases the patients were subject to hemorrhages, but these were not by any means restricted to the brain. They had epistaxsis, vicarious menstruation, etc. He does not call attention to any change in the blood-vessels.

Dr. SACHS: With regard to the case referred to by the President and Dr. Lloyd, I have simply to add that I do not doubt that meningeal hemorrhage is exceedingly frequent. I do believe, however, that a fair number of such cases are in reality cases of intracerebral hemorrhage, and there are, furthermore, a large number of cases of alcoholism, in which hemorrhage occurs, but in such cases as mine alcoholism would not apply.

With regard to the remarks made by Dr. Zenner, he has turned things upside down. I had weighed all points carefully, and had taken great pains to show that there were certain points in differential diagnosis as between hemorrhage and embolism, and I thought that all the facts pointed towards hemorrhage rather than embolism. The chief object of my paper was to call special attention to one point, and that is the *absence* of convulsions in these cases. My conviction is that presence or absence of con-

vulsions is a strong point in making a differential diagnosis between meningeal and intracerebral hemorrhage.

The Association then adjourned to meet on Friday morning.

Friday, third day, morning session.

The Association was called to order at 10.30 o'clock by the President.

Dr. BURT G. WILDER, of Ithaca, exhibited the medisected alinjected head of a murderer.

The features of the brain, which was thus hardened and exposed *in situ*, are to be discussed at a future meeting in comparison with another murderer's brain; the specimen is shown in illustration of the value of the method of *continuous arterial alinjection*, which has been applied to other heads, to brains, and to entire bodies (children, a chimpanzee, etc.), in the anatomical laboratory of Cornell University. The main features of the method are (*a*) the reception of the head within twenty-four hours; (*b*) preliminary washing out of the vessels with chloral (to which, perhaps, weak alcohol might be preferable; (*c*) continuous alinjection for a week; (*d*) continuity secured by a pressure of 80 *mm.* of mercury, which was reduced to 40 *mm.* when the flow became somewhat free; (*e*) gradual increase of the alcohol from 65 to 94 p. c.; (*f*) maintenance of a low temperature (8–11, C.); (*g*) accurate division of the head with a fine saw acting in a mitre-box; (*h*) the small cost. The injected alcohol represented 41.5 litres of 95 p.c., but about two thirds was regained, so at $3 per gallon it would cost $11, and at 75 cents (free of tax) only $3.

Dr. JAMES HENDRIE LLOYD, of Philadelphia, then read a paper entitled "Moral Insanity: A Plea for a More Exact Cerebral Pathology." The writer referred to John Locke's criticism of those who confound, in psychological analysis, the *agent* with the *powers* of the agent. This philosopher inculcates the individuality of the mind, and cautions against granting to the faculties each its own autonomy. The faculties "are not so many distinct agents in us, which have their several provinces and authorities, and do command, obey, and perform several actions, as so many distinct beings." It is this principle of abstracting the mere *qualities* or *actions* of a thing and then personifying them as distinct *substances* (against which Locke cautions) that the writer

proceeded to show had been the bane of the metaphysico-theological methods of studying mental functions. These errors have more than the mere speculative interest which attaches to the thoughts of the philosophers, because, as was shown, they have a practical application in almost every hospital and court-house of our land. They have crept into our text-books, confused our science, decided our practice, and unfortunately sometimes vitiated our testimony. The writer briefly alluded to Peats, and especially to the modern Scotch school of psychologists, for examples of the metaphysical imagination which has converted vast generalizations into potent personifications (or real existences), so that men have from them attempted even to construct a science of biology (as Plato's "ideas"), or have worshipped what has no substantive existence whatever. This tendency is shown by the Scotch school in their use of the words "consciousness" and "mind," which they abstract from the brain (of which they are, in reality, but the faculties and modes), and make distinct entities of them. The method of medical writers and alienists is too often inherited from the philosophers, and this is shown especially in their making of the faculties—judgment, emotion, and will—distinct beings, more or less independent of each other, and each having its own diseases. The writer taught that this was a vital error. These faculties are but different *modes* of action of one substance, which is the brain ; and are not localized in it but universal in it and all its sensori-motor centres. Reference to Carpenter and J. Stuart Mill brought out the distinction between the metaphysical and material views of relations of mind and brain. A brief review was made of this subject as it had been handled in the courts of law—where distinctions, affecting life and property, had been equally artificial and untrustworthy. The writings of Coke, Hale, Blackstone, Shelford, and Chitty exhibit the divisions into "total and partial" and "civil and criminal" insanity ; and the tests relied upon (knowledge of right and wrong and presence of delusion) were shown to be rooted in this wrong method of looking upon the mind as something capable of numerous divisions and subdivisions, some of which remained healthy while others were given over to ruin. The writer desired to make complete the illustrations of man's futile attempts to artificially construct our science. We must listen to kindred attempts often on the part of our own profession. An analysis was then made of the subject "moral insanity" as found in the

writings of Kuch, Prichard, Ray, Winslow, and Maudsley. Moral insanity proceeds upon an abstraction, just such as Locke warns his readers to avoid. It teaches that there is a moral "faculty" in the sense of a distinct agent, which has its own powers and its own diseases, and which may remain undeveloped in a "mind" otherwise healthy, or may become diseased without at all affecting the health of the other "faculties." It is nearer the truth to say that the whole brain-act of an insane man is wrong—judgment, emotion, memory, and will. To say that a man's intellect, for instance, is sound and his will diseased, is a sophism, which has more sound than reason, and is no better than to say that a Laclanché cell has electro-motive force but no current-strength. It is impossible to conceive of an emotional state of the cerebrum which does not include as essential the state also known as the intellect. Both intellect and emotion are but states or modes of the brain. It is not to the credit of psychiatry—which is the science of a diseased cerebrum—that this faulty method, inherited from the metaphysicians, should confuse its results. Moral insanity, and its big brood of special manias, is but the creature of bad science,—but the unfortunate insane, who are stigmatized by the term and robbed of sympathy and justice have, too truly, an existence. The subject was largely illustrated, and especially in conclusion, from the writings of the modern experimental physiologists, who are represented by Ferrier.

The PRESIDENT: I have great respect, both for the metaphysical and the practical knowledge of Dr. Lloyd, but at the same time I feel constrained to differ with him, with regard to some of the statements which, if I understood him correctly, he appears to make in his paper. Perhaps the difference between us, as is often the case, may be rather with reference to the use of terms and methods of expression, than a difference in point of fact.

The first general remark which I would make is that I do not see exactly what we are to gain, as practical physicians, from carrying out of the ideas expressed by Dr. Lloyd with reference to nomenclature and classification. What, after all, are some of the purposes of a classification, whether of diseases of the mind, or of any thing else? One of the purposes is to assist in the work of life, to help to clear the way for us, to aid us to understand more details with greater ease. The burden of the practical portion of the paper would seem to be that we should not separate moral insanity from intellectual insanity; that we

should not use terms of this kind ; that they are not only misleading, but actually injure the cause of science.

Then in his details he says that we cannot have a moral insanity without intellectual indisposition. Now any one who has made a practical study of insanity recognizes the truth of such statements, and therefore there is no difference in general terms ; and when we use such terms as monomania, moral insanity, kleptomania, etc., we do so understanding what we are doing, deprecating it perhaps, but doing it largely as a matter of convenience—the same as the scientist uses botanical or any other terms. Further, there is reason for the use and the existence of such terms as these, because the reports of cases are sufficiently numerous, and they have been seen by many of us in practice, to lead us to say that we have the right to believe that there are cases which deserve the designation of moral insanity, for the reason that we have in them an exemplification of a defect which predominates, sometimes exists almost exclusively, in the case.

The question of impairment of intellect is only a confusion of terms. Those who have used the term moral insanity are perfectly well aware that in every case, unless there is general paralysis, physical and mental, you must have exhibited more or less of what might be intellectual, as well as moral, impairment. Under this term only a certain number are included, such as cases which their whole lives have exhibited a preponderance of moral defect, and give decided evidence of what we know and recognize as moral impairment. In many of these cases you will find simply a little intellectual impairment, but we do find cases, however, where the moral impairment is so great, so extreme, and where the intellectual condition is so good, that, practically, using the terms in the way in which they only can be used, the insanity is only moral.

I believe that the term moral insanity, and there I agree with Dr. Lloyd in part, must be used with restriction, and perhaps some other term would be better. In the case of hysterical insanity you have moral depravity, etc., yet I would not rank it as either moral insanity or imbecility. The point which I wish to make is that we have just as good right, scientifically, to regard a case as one of moral insanity, as we have to use any one of the multitude of names which are used by all of us in describing spinal, or cerebral, or hepatic, or other diseases. I think that the Doctor puts himself into the same position as that occupied

by some of the metaphysicians whom he calls to an account. Now he speaks of disease of that unit the brain, and where does that lead us? It takes us way back to the days before Hippocrates, without any classification of insanity at all. Perhaps we have differentiated too much, but the Doctor's idea would simply turn back what has already been accomplished.

Dr. LLOYD: I distinctly stated in my paper that classification is necessary, and that differences must be recognized.

The PRESIDENT: I must contend that it is a fair inference, coming from the general argument in the Doctor's paper, when he says, with regard to insanity, that it should be spoken of as a disease of that unit the brain,—it is a fair inference that he will set us backward in this direction of scientific study.

While I cannot subscribe to all the views expressed by Dr. Lloyd, and while I believe that some of them will retard rather than assist experts and jurymen, I must pay a high tribute to the literary and scientific merit of his paper.

Dr. L. C. GRAY, of Brooklyn: I was glad to learn from Prof. Jastrow that metaphysicians are beginning to learn what they seem to have been oblivious to before. There is a book published by a physician of Edinburgh, in which he goes quite extensively into the subject of the modern experimentation with regard to localization, and leaves one to infer that it will be really a reunion of the practical physician and the abstract metaphysician, but when he draws his conclusion he is absolutely dominated by the old views of the metaphysical school, and the effect is to confirm all these old theories. I think while it is true of more metaphysicians than this one, still it is a dangerous thing to attempt to learn much from them. My ideal of a lecturer on moral philosophy would be first to make him a practising physician in mental and nervous diseases, presupposing that he should be thoroughly acquainted with the modern doctrines of localization, etc., and then, when his mind had become imbued with what are facts to a large extent, I would let him loose into the field of metaphysics. I think the trouble with most metaphysicians is that they study the moral aspect a great deal, while the physiological aspect receives only a very limited amount of attention. It is like studying the question of localization by *physiological* experiment alone; and we know how pathological data have modified the statements thus obtained.

I agree with Dr. Lloyd in one way and with Dr. Mills in another,

and I think that they do not disagree as much as they seem to. I think Dr. Lloyd means to state very clearly that we should not have the name moral insanity put upon these cases, and that if by the name moral insanity was understood simply moral depravity with intellectual disturbances, Dr. Lloyd and Dr. Mills would be in harmony.

As to the practical instances of moral insanity in which the moral symptoms are strongly predominant, and as to the existence of cases in which the moral symptoms are almost entirely intellectual symptoms, I have no doubt whatever, because I have seen such cases again and again. Suppose you take the case of a boy whom I saw some time ago, bright, quiet, intelligent, but taking pleasure while at the table in thrusting a fork into the chest of another boy. What is that but moral insanity? Take also a case reported by Hack Tuke, and one from an English asylum; I do not see how we are to classify such cases under any other head.

Of course a great deal of fuss has been made about the plea of moral insanity in the courts, but I do not think that any scientific body should be influenced by the courts, especially when such experts are brought into court as are frequently seen. Of course it is a very difficult thing sometimes to draw the line sharply between moral insanity and a case in which there may be a large amount of insanity, but in which the immoral symptoms are largely predominant. But practically, I think, according to my own experience, we should be at very great loss if we were to throw away so significant a term as this.

Dr. RALPH L. PARSONS, of Sing Sing: I suspect the particular trouble in the use of this term has been that it has in some way, in the popular and in the legal mind, become mixed up with the idea of responsibility and irresponsibility; and to the popular mind the term is objectionable because it means two different things; it may mean emotional insanity or it may mean simple wickedness.

With regard to moral insanity I am just as certain of its existence as I am of any of the other forms of insanity. We speak of different physical diseases, as disease of the liver, disease of the teeth, etc., but we do not mean that the patient is not a sick person. It is not simply the liver or the teeth, but the entire system which is affected. In a still stricter sense, I think we may say that there is a form of insanity which cannot be placed in any of our divisions of insanity, yet which exists really, but affecting the

other faculties of the mind more or less, whether the intellect, the emotions, or the will. Certainly we say that if a man wills, his mind is willing ; if he remembers something, it is his mind remembering : and that is very true. But with regard to mind remembering, he remembers certain classes of things, names, words, facts, mathematical truths, historical facts, better than some others.

My own convictions have been so well expressed by Dr. Mills and Dr. Gray that there is no necessity for extending my remarks, except simply to reiterate that in almost all forms of insanity with which I am acquainted, while there is sufficient distinction to call for classification, yet all the faculties of the mind are more or less involved.

The PRESIDENT : I would like to ask Dr. Lloyd two or three questions. I would like to ask him whether, in discussing the subject of insanity, we have any right at all to use such terms as moral or immoral ? Whether such terms should be employed in any sense ?

If so, in what sense should they be employed in discussing questions of this kind ?

Do not those who see cases of insanity frequently see those who present an overwhelming predominance of moral symptoms, of so-called moral or immoral phenomena ?

Now, then, suppose you have, in a genuine case of insanity, represented by ten, nine parts of moral defect and one of intellectual defect, where would he place the case ?

Dr. GRAY : I would also like to ask Dr. Lloyd a question. Why can't we have localization in regard to the action of the mind as much as we have central localization in purely somatic matters ? Why may we not have localized insanity by certain localized areas becoming involved ? What is there to prevent us from saying that it is possible ?

Dr. LLOYD : I feel much gratified at the politeness with which I have been treated. I can only say with reference to Professor Jastrow's remarks that I cannot get any satisfaction from the expression "line of least resistance." I prefer to say that I do not understand it. I do not believe that anybody does. How do we get an idea ? What is the exact condition of the cerebral matter when an idea is obtained ? I do not know. Nor do I believe any one else does.

With regard to moral insanity, all I can say is that it is more

than a mere difference in terms, and when the gentleman uses the term "moral" as distinct from "intellectual," and says that a man took a fork and stabbed another man, and took pleasure in it, and that he would call it moral insanity without disturbance of intellect, he speaks of something which has not been proved. Nobody has witnessed his mind with reference to the act. It is impossible to conceive of a boy stabbing another boy without cerebral intellection.

I think that it will be a sorry day when we get to despising our courts and our courts to despising us. I have studied law myself, and I do not propose to trifle with judges or juries. They have the right to insist that we should express ourselves, not paradoxically, but in intelligent, consistent language.

Dr. Mills asks what would you do with such cases, and where place the cases in which the bulk of the derangement is moral, or in which the bulk of that moral derangement is intellectual. I do not understand the moral to be apart from the intellectual. If the man's brain is affected, all the qualities of the faculties which come from it are also affected. Therefore I would say it is mere word differentiation, and not a differentiation of things. This is the only answer which I can give the question.

With regard to kleptomania or paranoia, I would be the last to say that such a person was *only* morally insane. The very idea involved is that the man has a congenital defect in his mental organization, and it has come from his mother's womb. Such cases derive their trouble from bad heredity.

With regard to classification I did not say that there is nothing in it ; but I do say that you must get at truths before you can classify them.

Dr. Geo. W. Jacoby, of New York, presented a new portable galvanic battery, described in a paper by Dr. J. Rudisch, of New York (see JOURNAL vol. xiii., p. 575).

Read by title : "Facts and Deductions Bearing on the Action of the Nervous System."[1] By F. X. DERCUM, M.D., of Philadelphia.

The following amendments to the By-Laws and the Constitution were affirmed :

By Dr. R. W. AMIDON, of New York : "The officers shall enter upon their duties immediately after the adjournment of the annual meeting at which they are elected."

[1] Will appear in JOURNAL, Nov., 1886.

By Dr. C. K. MILLS, of Philadelphia: "Two Vice-Presidents instead of one Vice-President."

By Dr. G. W. JACOBY, of New York: "They (officers) shall be nominated by the Association on the *second* day of the annual meeting" (instead of the first day).

"There shall be two sorts of members, namely, active members —not exceeding at any time one hundred in number" (instead of fifty as at present).

Resolutions.

Dr. L. C. GRAY, of Brooklyn, introduced a resolution endorsing the proposition of the proposed "Congress of American Physicians and Surgeons," and moved that a committee of conference of five be appointed by the President. The resolution and the motion were adopted.

The PRESIDENT appointed Drs. L. C. Gray, of Brooklyn; J. Van Bibber, of Baltimore; W. Sinkler, of Philadelphia; E. C. Seguin, of New York; and Philip Zenner, of Cincinnati.

Officers for the Ensuing Year.

President.—L. C. Gray, of Brooklyn.

Vice-President.—John Van Bibber, M.D., of Baltimore.

Secretary and Treasurer.—G. M. Hammond, M.D., of New York.

Councillors.—B. Sachs, M.D., of New York, and Wharton Sinkler, M.D., of Philadelphia.

The Association adjourned to meet in June, 1887, the date and place to be designated by the Council.